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Biogeosciences
Discussions

Interactive comment on "Effect of CO_2 on the properties and sinking velocity of aggregates of the coccolithophore *Emiliania huxleyi*" *by* A. Biermann and A. Engel

Anonymous Referee #3

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Biermann and Engel address potential changes in properties and sinking velocities of coccolithophore aggregates in response to ocean acidification. This is a topic of high relevance. The authors present results from a set of carefully conducted lab experiments. Cultures from a single strain of Emiliania huxleyi were subjected to three different CO2 treatments. Aggregation was initiated after transfer of subsamples to roller tanks. A suite of chemical measurements allowed for the characterization of aggregates, their sizes, compositions, sinking velocities were assessed. The authors use these results to infer changes in the biological pump under future high CO2 conditions. I am puzzled by the discussion and the conclusions. It is not clear to me where and how this study goes beyond the state-of-the-art. The effect of calcite on excess density

and hence sinking speed was already documented by Engel et al (2009). The impact of mineral ballasting on aggregate properties including size and sinking velocity is also addressed in Passow and DeLaRocha (2006). I am concerned by the in general inappropriate consideration of published results from other groups (e.g. Passow et al, Ploug et al). This shortcoming explains part of the weakness of the discussion.

The discussion of the future evolution of the biological pump is highly conjectural and needs to be carefully reworked. The authors mention the changing global C cycle as a motivation of their study. What would be the consequences to the C cycle of the suggested changes in export efficiency?

The controls of particle fluxes are still poorly understood. One important aspect of this study is its potential contribution to a better quantitative understanding of the relationship between particle composition and sinking velocity. Up to now model based projections of future changes in marine carbonate geochemistry focused on carbonate production and dissolution. The decrease in the penetration depth of POC fluxes linked to a weaker ballasting has so far not been addressed in a satisfying manner. This is in part due to the lack of experimental data and appropriate empirical relationships. In that respect, I am intrigued by the lack of relationship between size/mass and settling velocity for HCT and the weak one for MCT. Is excess density the main control? And how does this compare to Passow and DeLeRocha (2006)?

Specific comments: 1) use of a single strain of E. huxleyi: Langer et al (2009) suggest that the variability in responses of PIC and POC production to increasing CO2 in Emiliania huxleyi reported in the literature might be due to genetic differences between strains. While I don't suggest repeating the experiments with different strains, this point should be acknowledged in the discussion. 2) carbonate chemistry: I appreciate that the CO2 system was manipulated by bubbling of a gas mixture. The authors need to specify on which scale pH values are reported, as well as which dissociation constants were selected for the CO2SYS calculations and finally what stoichiometric solubility products was used for calcite. In general, the analytical section should be completed by information on calibration procedures, accuracy and precision. 3) acclimatization of organisms to experimental conditions: I understand from the method section that the cultures were not acclimated to the different CO_2 levels. This could represent an important bias and needs to be acknowledged and discussed. I wonder to what extent it could explain part of the differences between treatments. Could it be the HCT reflects a response dominated by stress? Was the production of TEP measured during the experiments? These data would complement the existing set of results. I expect significant differences between treatments, with perhaps highest values during HCT. 4) comparison to previous studies: the authors use PIC to POC ratios published for natural assemblages to infer the validity of their experimental results obtained for monospecific cultures of Emiliania huxleyi. This is quite confusing and might lead the reader to the wrong conclusion. Similarly, results from lab studies and observations of natural aggregates are mixed when discussing sinking velocities and the importance of composition and size. I am not criticizing the comparison between results from different approaches, this should be done, I am recommending greater care. 5) viral infection: the last paragraph is pure speculation! This leads me to a final comment: please focus on what you have actually measured and avoid presenting inferences as hard evidence, when you have no measurement (bacterial activity or respiration, viral infection etc).

Despite my critics, I still believe that there are sufficient data in this study to make up for a valuable contribution to the field after major revisions. I strongly encourage the author to resubmit a revised version.

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